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DARWIN-WALLACE CENTENARY CELEBRATIONS
July, 1958
AND LINNAEUS' *SYSTEMA NATURAE* BI-CENTENARY

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DARWIN-WALLACE CENTENARY CELEBRATIONS
1 July 1958

At a Meeting of the Linnean Society held on 1 July 1958, attended by members of the Darwin and Wallace families, representatives of other societies and institutions, and members of the Linnean Society, the President, Dr C. F. A. Pantin F.R.S., unveiled a plaque in the Meeting Room commemorating the centenary of the reading before the Society on 1 July 1958 of the joint communication by Charles Darwin and Alfred Wallace on their theory of Evolution by Natural Selection. The plaque reads:

CHARLES DARWIN
AND ALFRED RUSSEL WALLACE
MADE THE FIRST COMMUNICATION
OF THEIR VIEWS ON
THE ORIGIN OF SPECIES
BY NATURAL SELECTION
AT A MEETING OF THE LINNEAN SOCIETY
ON 1ST JULY 1858 1ST JULY 1958

In further commemoration of this Centenary the Society held a Dinner in the Draper's Hall on the evening of 1 July 1958 attended by members of the Darwin and Wallace families and Fellows and their guests.

PRESIDENT'S ADDRESS AT THE UNVEILING OF THE DARWIN-WALLACE MEMORIAL PLAQUE

The President said :—

Before I give my address I particularly want to thank on your behalf one of our Associates, Mr Gerald Atkinson who has cleaned and restored our fine picture of Charles Darwin. We thank him most heartily for what he has done and that we see the picture so well to-day.

Here in our meeting room we have set up a plaque to mark the centenary of the most important event in the history of our Society since its foundation. Exactly one hundred years ago, on 1 July 1858, a paper was read before the Society :—

> ' On the Tendency of Species to form Varieties ; and on the perpetuation of Varieties and Species by Natural Means of Selection.' By Charles Darwin, Esq., F.R.S., F.L.S. and F.G.S. & Alfred Wallace, Esq.

Neither author was present and the paper was communicated for them by Sir Charles Lyell and J. D. Hooker.[1]

[1] Darwin, C. & Wallace, A. 1858. *J. Linn. Soc.*, **3** ; 45.

That paper was the first act in one of the most dramatic intellectual revolutions that ever happened to mankind. I wish we could say that it was here in this room that the paper was read. But our former meeting room no longer exists. It was then situated at the east end of what are now the rooms of the Royal Academy ; and owing to later reorganization the site is now occupied by the stairway and lift to the Diploma Gallery, a pointless and unsuitable place for a commemorative tablet. So we cannot say, ' Here is the very room where it happened ', any more than we can go to the Museum at Oxford and say, ' Here is the very room where Thomas Henry Huxley and the Bishop of Oxford opened the great Victorian debate on the Evolution of Man '. More's the pity. All the same, it is in this place, where we meet together, that we should remember what Darwin and Wallace did.

The publication of their paper stands in the middle of the last century, between the old and the new science. It does so almost literally. In the recorded *Proceedings* of that year there are three papers in succession. The first is by Thomas Huxley on the anatomy of *Nautilus pompilius* ; that extraordinary palaeozoic survival and relative of the squids and octopus of to-day and of the Ammonites of yesterday. All that paper needs is a touch of phylogenetic interpretation to make it a characteristic contribution to evolutionary morphology by the man who became a master of it. The last paper of the three is by Dr Robert Knox on the classification of the Cetacea, in which he separates the Sirenia, the toothed whales and the whale-bone whales. It was almost his last major scientific contribution. His name carries us back to the grand days of the Edinburgh Anatomy School and, tragically and horribly, to the resurrectionists and the murders by Burke and Hare. He might have been one of our great comparative anatomists, but he was at least the teacher of Charles Darwin's opponent, Richard Owen.

Between Huxley and Knox comes the contribution of Darwin & Wallace. On an occasion like this I know you will not ask me to apologize for going over that old story ; even though in this year men much more competent than I have already done so and will do so again. As a background to the paper let me begin by two quotations. First from Darwin's autobiography :—[2]

> In October, 1838, that is, fifteen months after I had begun my systematic inquiry, I happened to read for amusement, " Malthus on Population," and being well prepared to appreciate the struggle for existence which everywhere goes on from long-continued observations of the habits of animals and plants, it at once struck me that under these circumstances favourable variations would tend to be preserved, and unfavourable ones to be destroyed. *The result of this would be the formation of new species.* Here, then, I had at last got a theory by which to work ; but I was so anxious to avoid prejudice that I determined not for some time to write even the briefest sketch of it. In June, 1842, I first allowed myself the satisfaction of writing a very brief abstract of my theory in pencil, in thirty-five pages, and this was enlarged during the summer of 1844 into one of 230 pages.

And then from Wallace :—[3]

> In February, 1858, I was suffering from a rather severe attack of intermittent fever at Ternate, in the Moluccas ; and one day, while lying on my bed during the cold fit, wrapped in blankets though the thermometer

[2] Barlow, N. (edited by). 1958. *The Autobiography of Charles Darwin* 1809–1882. London.

[3] Wallace, A. R. 1908. *My Life.* London.

> was at 88° Fahr., the problem again presented itself to me, and something led me to think of the "positive checks" described by Malthus in his "Essay on Population", a work I had read several years before, and which had made a deep and permanent impression on my mind. These checks—war, disease, famine and the like—must, it occurred to me, act on animals as well as man. Then I thought of the enormously rapid multiplication of animals, causing these checks to be much more effective in them than in the case of man; and while pondering vaguely on this fact there suddenly flashed upon me the *idea* of the survival of the fittest—that the individuals removed by these checks must be on the whole inferior to those that survived. In the two hours that elapsed before my ague fit was over, I had thought out almost the whole of the theory; and the same evening I sketched the draft of my paper, and in the two succeeding evenings wrote it out in full, and sent it by the next post to Mr. Darwin.

How like those two passages are, even though these two men were very different both in character and in their scientific method of attack, But, as naturalists, both had abundant and direct contact with natural phenomena in the field and respect for such phenomena. Both had read Malthus. Independently, they both arrived at exactly the same argument. The potential rate of multiplication of living things is enormous; but their population is generally stationary, so that few can survive. Animals and plants vary; and some varieties will be better adapted to the environment than others. The small proportion of survivors in each generation will tend to be those varieties best adapted to the environment; for these will have the greatest chance of survival. Variation tends to be inherited; so that there is here a natural means of selection in favour of adaptive evolution further and further from the original type.

Julian Huxley has pointed out the essentially deductive character of the argument.[4] Like all deduction it is absolutely conclusive if the premisses are correct. Unlike the wild assumptions of Lamarck and other early evolutionists, the premisses of Darwin and Wallace's argument were ideas commonly accepted by both naturalists and breeders from their own experience. That was the immense strength of their theory; deductive argument based on commonly accepted ideas. But before Darwin and Wallace no one had shown precisely what these ideas implied.

You will remember how Darwin in London received Wallace's essay from Ternate. And how at first he wished to give it priority of publication over his own long digested conclusions. And how in the end he let Lyell and Hooker take the matter out of his hands and present the joint communication. That was honourable to Darwin and still more honourable to Wallace. The action of Lyell and Hooker was absolutely right. To-day, priority of publication has become so firmly knit to professional advancement and prestige, that it is seen as a calamity to be second when two men reach the same scientific conclusions in quick succession: so, like the gold-diggers, we hasten to stake our claim in the fields of nature. But what can possibly be better for the advancement of human wisdom than that two men should independently arrive at the same conclusion? What can more clearly show that we are on the road to truth? Of course from our human point of view the question is not simple. Because of the frailty of our nature we cannot avoid ambition; and though not a noble character of our species it can be a spring of action towards noble things. If Lyell had not applied the whip of ambition to Charles Darwin, should we have had the *Origin of Species* in 1859?

[4] Huxley, J. 1942. *Evolution, the modern synthesis*. London.

But Wallace is a fine example for us to-day. He promptly and sincerely gave Darwin pre-eminence ; and it was no grudging gift made for conscience sake. He called himself ' a very bad second in the truly Olympian race '. Second he was to Darwin—but the rest of the field was nowhere. And when all is said and done he was not a bad second at all, but equal first in the first event : the independent discovery of the principle of Natural Selection. The fact that Darwin in October 1838, was the first to think of it and that Wallace in February 1858, was the first to offer it for publication, is not really important.

The events following the communication of the Darwin and Wallace paper are most interesting. When we read it to-day what is said seems obvious enough : for this tremendous novelty is now incorporated in our system of everyday accepted beliefs. That it should have so little apparent effect at the time may seem strange. In spite of the interest there was no attempt at discussion of the paper. Professor Haughton of Dublin said of the communications that all that was new in them was false, and what was true was old.[5] And at the Anniversary meeting next year on 24 May 1859, Thomas Bell, our President, reviewing the work of the society, missed his cue in the grand manner :—

> The year . . . has not, indeed, been marked by any of those striking discoveries which at once revolutionize, so to speak, the department of science on which they bear ; it is only at remote intervals that we can reasonably expect any sudden and brilliant innovation which shall produce a marked and permanent impress on the character of any branch of knowledge, or confer a lasting and important service on mankind. A Bacon or a Newton, an Oersted or a Wheatstone, a Davy or a Daguerre, is an occasional phenomenon, whose existence and career seem to be especially appointed by Providence for the purpose of effecting some great important change in the condition or pursuits of men.[6]

Even Thomas Huxley did not see what had happened. In June 1859, he gave a lecture before the Royal Institution on the geologically ' Persistent Types of Animal Life '.[7] He remarked :—

> It is difficult to comprehend the meaning of such facts as these, if we suppose that each species of animal and plant, or each great type of organization, was formed and placed upon the surface of the globe at long intervals by a distinct act of creative power ; and it is well to recollect that such an assumption is as unsupported by tradition or revelation as it is opposed to the general analogy of nature. If, on the other hand, we view ' Persistent Types ' in relation to that hypothesis which supposes the species living at any time to be the result of the gradual modification of pre-existing species a hypothesis which, though unproven, and sadly damaged by some of its supporters, is yet the only one to which physiology lends any countenance . . .

There you still have it—' Unproven . . . yet the only one to which physiology lends any countenance.'

But the curious latent period soon ended and then the rate of changes of ideas with time underwent a sharp increase. Professor Alfred Newton, the Cambridge ornithologist, found Darwin & Wallace's paper on his return from a trip to Iceland. He read it straight through and found, as he says, ' a per-

[5] Barlow, N. (edited by). 1958. *The Autobiography of Charles Darwin* 1809–1882. London.

[6] Bell, T. 1859. Anniversary meeting. *Proc. Linn. Soc. Lond.* 1858–59, viii.

[7] Huxley, T. H. 1859. *Proc. Roy. Inst. Gt. Brit.*, **3**,; 151.

fectly simple solution of all the difficulties which had been troubling me for months past '.[8]

Then, on 24 November 1859, was published the *Origin of Species* : the first edition of 1250 copies was sold the same day and by 1876 no less than 16,000 copies had been sold in England alone.[9] What happened has been likened to an explosion. It is indeed quite proper to borrow similes from physical chemistry; but the event was not so much like the conducted disturbance of an explosion as the almost simultaneous activation of an entire mass in a detonation. The effect of Darwin's book on Thomas Henry Huxley was as a flash of light :— ' How extremely stupid not to have thought of that !'[10]

But to the theory of Natural Selection set out by Darwin & Wallace the *Origin of Species* had added a tremendous new element. It might seem that the deductive argument of natural selection based on commonly accepted ideas had provided all that was needed for conviction of the truth of evolution. But mankind has a great distrust of logical argument. This often vexes the theorist who then accuses his antagonists of lacking the power of reason. Nothing can excite his opponent more than that ; whence there is anger, and much subjugal heat. It is, of course, not the logic which is distrusted at all : it is the assumptions. Bluntly the ordinary man usually feels that he is not clever enough, and that the clever theorist may yet be too ignorant, to know whether the assumptions really cover the case. And in feeling this he is often quite right.

Consider the controversy over the age of the earth. In the latter part of the 19th century the immensity of geological change was making the geologists demand whole ages of time. Then Lord Kelvin abruptly took them to task and told them that since the beginning of the world they could have one hundred million years and not a day more. He based his calculations chiefly on the gravitational energy of a cooling sun. The geologists wanted much more time than that : Charles Darwin himself had even indented for upwards of three hundred million years since the latter part of the Mesozoic.[11] Huxley vigorously defended the geologists' demands. But the physical forces were strong and several ' yes-calculations ' from the amount of sodium in the ocean and so on agreed remarkably with Kelvin's estimate.[12] And then one day we were told, so to speak, ' Delete one hundred and insert three thousand million because of radioactivity. Signed—E. Rutherford.' And the geologists said, ' I knew there was a catch.'

Something of the same sort seems to be happening to-day about the Wegner theory of continental drift.

To make mankind believe a hypothesis—to give a belief what John Henry Newman calls ' assent '—we must not only show that it is logically unassailable but that it is in fact congruent with the great edifice of existing knowledge. One hundred years ago the notion of the fixity of species was ' built in ' to the whole structure of biology and geology, not to mention other parts of knowledge. Could the whole of this great structure withstand the introduction of the idea of evolution in the place of fixity without raising even greater difficulties than would be caused by the denial of Darwin & Wallace's logical theory of Natural Selection? Could the accepted notions about variation, inheritance and selection on which that was based be substantiated by a really formidable body of circumstantial evidence?

[8] Seward, A. C. (edited by). 1909. *Darwin and Modern Science*. Article by W. Bateson. Cambridge.
[9] Darwin, F. (edited by). 1887. *Life and letters of Charles Darwin*, **1**. London.
[10] Huxley, L. (edited by). 1900. *Life and Letters of Thomas Henry Huxley*, **1**. London.
[11] Thompson, S. P. 1910. *The Life of Lord Kelvin*, **1**. London.
[12] Joly, J. 1915. *The Birth-time of the World*. London.

It was exactly the necessity of convincing people about this which Darwin's genius foresaw and which the enormous labour of 20 years enabled him to meet with precision in the *Origin of Species*. This part of the work is to Darwin's credit alone, and because of it he must be placed even above Wallace.

I have spoken about the 'Origin' because I want to make quite clear what I believe to be the distinction between what was Darwin's contribution in that book and what was his and Wallace's contribution in the paper delivered here 100 years ago. The publication of the 'Origin' was the great and essential second step. But the brilliant enunciation of the principle of natural selection made to this Society was the first step in the scientific revolution. Natural Selection is in fact one of the great principles governing the natural world. It transcends even biology itself; for in its most general form it is one of the major laws of thermodynamics governing the fate of matter and energy in complex systems.

The principle of Natural Selection was seen by Darwin in 1838 while he was just under 30 years of age, two years after the young naturalist had returned from his five-year voyage on the *Beagle* on which he embarked at the age of 22. It was perceived by Wallace in 1858 when he was just 35, after starting on the life of a naturalist at the age of 25 and spending nearly ten years in the Amazon and the Malay. We may remember of Linnaeus himself that he said of his great works that he 'had thought out everything before his twenty-eighth year'.[13] The great ideas grew from young men, naturalists, in their twenties. That these things are really the work of young men is important.

It has become the fashion to subject the personalities of eminent Victorians to analysis. They have done that to Darwin. What is said about his invalidism, his hypochondria and his immaturity is of great interest. Darwin was a very unusual man, and the means by which he ensured that his way of life allowed him to do his work and evade committees and all the rest of it were as highly successful as they were odd. If he was neurotic, he used his neurosis as a powerful instrument to perform a task which possibly could have been done in no other way. Are we perhaps being just a little silly if we speak in the same breath of Darwin's maladjustment and of his remarkable power of adaptation to the conditions of his existence?

But apart from all this, to an ordinary biologist the usual question comes to mind: 'Are you sure you have looked at the whole specimen?' There is much more to Darwin than the 'life of the shawl' of his later years, and more to him than the rather shadowy figure of the boy we see through the old man's eyes. The young explorer of 22 did something which does not pale beside what men do to-day. He spent five years going round the world in the straitened and severe conditions of a ten-gun brig of the last century. He travelled extensively in a country which was not tamed. There were no sulphonamides, no radio and no helicopters to get him out of his difficulties. And during the course of those travels this boy laid the foundations not only of the theory of Natural Selection and of the *Origin of Species* but also of his brilliant geophysical theory of the origin of coral reefs: a theory which has survived the repeated onslaughts of its rivals. The best of our young men of a like age to-day have just sent in buff forms to our committees. The prize we offer them is three more years of tuition and tutelage towards the degree of Doctor of Philosophy. When they say Charles Darwin's personality is immature what do they really mean? Are they not succumbing to that besetting danger by which the specialist of the descriptive sciences takes the common terms of our language with all their accepted associations and imposes on them a specialist meaning of his own?

[13] Uggla, A. H. 1957. *Linnaeus* (translated by Alan Belair). Uppsala.

The plaque immediately after the unveiling by the President.

Charles Darwin's voyage which enabled him to make the contribution we commemorate shows the man quite as much as does his later life. What he did was in the same tradition and bore the same hazards as those disciples of Linnaeus, of whom more than one lost his life in exploration: Hasselquist, Forsskål and Löfling.

Given the quality of the man, the wealth of Darwin's father enabled him to do as he did. But before we beguile ourselves with the notion that in the 19th century opportunity was only open to the rich gentry let us remember Michael Faraday and Alfred Russel Wallace.

> This mournful truth is ev'rywhere confessed:
> Slow rises worth by poverty depressed.

But rise it did in these young men as well as in Samuel Johnson. Wallace was an almost penniless young land surveyor. The railway boom of the 1840's just enabled him to scrape together £100, and with that sum he set out with Bates on a grand expedition to the unexplored regions of the Amazon. Whilst returning he suffered the loss of almost all his specimens in shipwreck. He spent ten days in an open boat in mid-Atlantic, and but for good fortune would have lost his life. Undeterred he soon set out again, this time to the Malay Archipelago. In the comfort of to-day's travel one of the great thrills for the biologist is to look down from an aeroplane and see the islands of Bali and Lombok, the two sides of 'Wallace's line'. It was from Ternate in those parts that he made his contribution to our *Proceedings*.

What led such very different young men as Wallace and Darwin to reach identically the same theory of Natural Selection, and to reach it alone among their contemporaries? Alfred Wallace himself discussed that at our Jubilee celebration of fifty years ago.[14] He notes that both of them in early life had been ardent beetle-hunters and reminds us of the wealth of structural diversity and adaptation in these insects. He stresses what is underrated to-day, 'the mere passion for collecting'. These are his words:—

> I should describe it rather as an intense interest in the mere variety of living things—the variety that catches the eye of the observer even among those which are very much alike, but which are soon found to differ in several distinct characters. Now it is this superficial and almost child-like interest in the outward forms of living things, which though often despised as unscientific, happened to be *the only one* which would lead us towards a solution of the problem of species. For nature herself distinguishes her species by just such characters—often exclusively so, always in some degree—very small changes in outline, or in the proportions of appendages, as give a quite distinct and recognizable *facies* to each, often aided by slight peculiarities in motions or habits; while in a large number of cases differences of surface-texture, of colour, or in the details of the same general scheme of colour-pattern or of shading, give an unmistakable individuality to closely allied species.

It is not enough to treat natural phenomena as mere isolated objects provided for the testing of our hypotheses. It is when you really know your material —and what is more have come to love it—that you really begin to see and to perceive new relationships. That is as true to-day of the pictures taken under the electron microscope as it is of species in the field.

With that sort of intimate knowledge of species and their life, it then happened that both Darwin and Wallace read Malthus. These were the primary

[14] Linnean Society of London 1908. *Darwin-Wallace Celebration 1st July* 1908. London.

requirements. But in addition Alfred Wallace attached great importance to the large amount of solitude he and Darwin both enjoyed when they were young men on their travels at an impressionable period of their lives. That is I believe a necessary condition for what Samuel Taylor Coleridge calls ' that willing suspension of disbelief for the moment ' that precedes the highest acts of the imagination.[15] Without that it is not possible to reassemble in a new coherent pattern the great set of images by which we represent both the natural and the human world to our minds : without it neither Wallace nor Darwin could have seen the fixed species to be mutable.

All that is a young man's game. The old man in the shawl on his couch dutifully performed the tasks the young man had set him. The other young man, Alfred Wallace, with his different social circumstances and his genius for bad finance could never give these same advantages to his older self. Lack of means may nip the fruit more than the bud.

I say these things because this tablet to Charles Darwin and Alfred Russel Wallace does much more than mark a famous and honourable event in the history of our Society. It is a reminder that in the end our scientific greatness, and indeed all our greatness, depends not upon steering committees but upon the young men and the young women and on their freedom of thought and freedom of action.

I hope that from time to time those who direct the future of our young men and women may see this tablet—and our fine picture of the old Charles Darwin above it. And I hope that their mind's eye can read a motto beneath it by yet another young man who never sat for an honours degree :—' Youth's a stuff will not endure '.

LINNAEUS' *SYSTEMA NATURAE* BI-CENTENARY AND DARWIN-WALLACE CENTENARY CELEBRATIONS

15 July, 1958

SPECIAL MEETING

On 15 July at a Special Meeting, held in the Memorial Hall of the Royal Geographical Society, Silver Darwin-Wallace Medals were presented to 20 British and Foreign Biologists in recognition of their outstanding contributions to our knowledge of Evolution, and in the evening a Joint Conversazione of the Royal, Geological and Linnean Societies was held at Burlington House, when a film by Dr H. B. D. Kettlewell entitled ' Darwin and the Insect Adaptations of Brazil ' and exhibits relating to Darwin and Wallace were shown.

A Darwin-Wallace Commemoration Number of the Society's Journals was issued on 1 July (*Botany* No. 365 and *Zoology* No. 295) containing contributions by :—Sir Julian Huxley, F.R.S.; Sir Gavin de Beer, F.R.S.; Prof. O. M. B. Bulman, F.R.S.; E. J. H. Corner, F.R.S.; Dr E. B. Ford, F.R.S.; Prof. R. Darnley Gibbs ; Dr S. M. Manton, F.R.S.; Prof. I. Manton ; Dr L. Harrison Matthews, F.R.S.; Dr F. R. Parrington ; Prof. G. Pontecorvo, F.R.S.; Dr H. Hamshaw Thomas, F.R.S. and Dr W. B. Turrill, F.R.S.

[15] Coleridge, S. T. 1817. *Biographia Literaria.* Cap 14. Everyman Edition, 1947. London.

PRESIDENT'S ADDRESS BEFORE THE PRESENTATION OF THE SILVER DARWIN-WALLACE MEDALS LIST OF RECIPIENTS AND CITATIONS

Before we begin this our Special meeting I have the very pleasant duty of welcoming the many distinguished guests whom we find with us here to-day. Let me tell them how very glad we are to see them.

The occasion of this Special meeting of our Society is a double Centenary. It is just 200 years since the publication of the 10th edition of the *Systema Naturae* of Linnaeus. On that is based the whole of our present classification of the Animal Kingdom. Again on the first of this month at Burlington House we marked the centenary of a famous scientific event. A hundred years ago a paper was read before our Society :—

> On the Tendency of Species to form Varieties ; and on the perpetuation of Varieties and Species by Natural Means of Selection.

That was the first statement of the principle of Natural Selection ; and the statement was independently made by Charles Darwin and Alfred Russel Wallace.

The consequences of that discovery in every part of biology and indeed in the whole of human thought have been immense. To commemorate it, we have with us to-day leading biologists, whose work has been built upon that of Darwin and Wallace ; and to these and to certain others who cannot be with us we wish to offer a signal mark of our appreciation of what they have done.

But first, I wish to draw your attention to certain important things about that discovery. To-day we are apt to identify scientific advance with the ordered development of successful theory. That view is perhaps influenced by the spectacular advance of the physical sciences and the development of their technical applications. I certainly do not belittle that side of scientific advance ; indeed the ordered development of biology since the *Origin of Species* is a profound illustration of its importance. But the advent of Natural Selection reminds us of two other components in the major developments of science. One is the unexpected consequences that may follow when brilliant minds are excited by the international exchange of ideas. The other is the immense importance of direct contact with the whole array of natural phenomena in the field, as distinct from the few facts which we in our white coats select for special investigation in the laboratory.

To take the first case : curiously and quite indirectly we may be said to owe the discovery of Natural Selection to that extraordinary man Jean Jacques Rousseau. His romantic notions and those of his English follower William Godwin about the perfectability of man were welcomed by many in the latter part of the 18th century. One who received these ideas with generous optimism was Rousseau's friend and correspondent—apparently one of his executors—Daniel Malthus. But the bright mind of Daniel's son Thomas Robert Malthus saw the essential weaknesses that underlay the emotional assumptions of the romantic reformers. Encouraged by his father he wrote his famous *Essay on Population*. There he showed the dependence of human welfare on the balance between excessive reproduction and the restrictions of available food and other checks. It was in turn this argument of Thomas Malthus which quite independently flashed across the minds of Darwin and of Wallace and gave them each the key to the principle of Natural Selection. Thus indirectly and unpredictably do the really great ideas often arise.

That the inspiration of both Darwin and Wallace depended on their abundant contact with the rich variety of natural phenomena is clear enough from their journals. The way in which such naturalists went throughout the world to

collect information and specimens is in the great tradition of the man whose name our Society bears :—Linnaeus. Charles Darwin as naturalist on the *Beagle* followed Linnaeus's own disciples Solander and Sparrmann who sailed with Captain Cook to Australia, New Zealand and the South.

We may note that when Charles Darwin sailed in 1831 he could not then have filled in some appropriate form of application by stating ' Object of the Expedition :—The discovery of the origin of species '. It is essential that in addition to the analysis of isolated phenomena in our laboratories men should from time to time go out with their wits about them to view the natural world as a whole. It is, therefore, particularly encouraging that this autumn the Royal Society is sending a pilot expedition, which will include British, New Zealand and Chilean men of science, to explore the biologically important and rapidly changing regions of southern Chile down to Tierra del Fuego—some of Charles Darwin's own ground.

But apart from all this, the discovery of Natural Selection by Darwin and Wallace was rendered possible by the brilliant work of Linnaeus which enabled us to classify and set in their right order the kingdoms of living things. Accurate identification of the things we talk about is essential to good science. The theory of evolution depended on the work of Linnaeus ; and the principles of taxonomy were in turn revolutionized by the advent of evolution. To-day we have two communications before us dealing with these matters. Before we receive those communications it is my pleasant duty to tell you briefly of those men whom we wish to-day to honour by the gift of the Darwin-Wallace medals.

These are they :—

Dr Edgar Anderson, greatly distinguished for his work on plant genetics, particularly for his successful combination of experimental research and field studies in elucidating the composition and organization of wild populations.

Professor Maurice Jules Gaston Corneille Caullery, F.M.L.S., our old friend and Linnean Medallist ; doyen of all zoologists, who has made lasting contributions to our knowledge of all branches of the subject, particularly parasitism and symbiosis.

Sir Ronald Aylmer Fisher, already in his lifetime the classic authority on the application of statistics to genetical problems and author of the genetical theory of Natural Selection ; to him is due the integration of Darwinian selection and Mendelian genetics, the essential machinery of evolution.

Professor Dr Carl Rudolf Florin, so well known to us for his distinguished work on palaeobotany and particularly for the light he has shed upon the phylogeny of the Gymnosperms.

Professor John Burdon Sanderson Haldane, biochemist and geneticist, pioneer of that application of mathematics to the problems of selection and to population studies which has done so much to clarify the process of evolution.

Professor Roger Jean Heim, F.M.L.S., who has made such great contributions to the science of Mycology, particularly in relation to the phylogeny of the higher fungi ; and who is well known to us for his active concern in the preservation of nature.

Dr John Hutchinson, so distinguished for his work on the taxonomy of the flowering plants ; and for the application of phylogenetic principles to taxonomy.

Sir Julian Sorell Huxley, author of the greatest synthesis on evolution, based on his own observations and experiments ranging over the fields of genetics, embryology, sexual selection, growth and systematics, and made vivid with his insight.

Professor D. M. S. Watson, F.R.S., replying on behalf of the Zoological Recipients of the Darwin-Wallace Medal

Dr Ernst Mayr, F.M.L.S., that leading student of the relation of species-formation to geographical isolation, to genetics, selection and ecology, and of the species-problem in systematics.

Professor Hermann Joseph Muller, foremost authority on the phenomenon and problems of mutation and one of that great team of Thomas Hunt Morgan, which made genetics a science.

Academician Yevgainee Nickonorvich Pavlovsky, pioneer in the study of evolution of infectious and parasitic diseases in plants, in animals and in man, and originator of the theory of natural foci of diseases as biocoenoses.

Professor Dr Bernhard Rensch, F.M.L.S., the pioneer of the study of species-formation in relation to geographical distribution ; it is to him that we owe the principle of the *Rassenkreis*.

Dr George Gaylord Simpson, F.M.L.S., who has shown us how palaeontology can inform us about variability, generation-length, and the time scale of evolution, and how it can be interpreted in the light of Natural Selection.

Professor Carl Johan Fredrik Skottsberg, F.M.L.S., who has made such manifold contributions to botany, especially in the fields of taxonomy and plant geography, and upon the interaction and interrelation of these disciplines.

Professor Erik Andersson Stensiö, F.M.L.S., Linnean Medallist, who notwithstanding the imperfection of the fossil record has demonstrated to us the anatomy of fossil animals in the most minute detail, thereby greatly extending and refining our knowledge of evolution, particularly in the lower Vertebrates.

Dr Hugh Hamshaw Thomas, past President of the Linnean Society, whose fine work on palaeobotany especially in relation to the early history of the Angiosperms, has often been laid before the Society.

Professor Dr Göte Vilhelm Turesson, from whose distinguished contributions to gene-ecology the modern experimental taxonomy of higher plants has arisen.

Professor Dr Victor Van Straelen, F.M.L.S., past Director of the Royal Institute of Natural Sciences of Belgium, and originator of the *Service Educatif* : under whose inspired leadership all branches of natural history have prospered exceedingly, not least from his outstanding contributions to the conservation of nature and his own researches in palaeontology.

Professor David Meredith Seares Watson, Linnean Medallist, for two generations acknowledged master in the study of vertebrate evolution through his own brilliant researches in palaeontology and the interpretation of those researches in the light of functional morphology.

The late **Dr John Christopher Willis,** whose novel and stimulating contributions to plant geography, so well known under the title of *Age and Area,* have been brought to an end by death since the Society announced this award to him.

LINNAEUS *SYSTEMA NATURAE* BI-CENTENARY ADDRESSES

Following the presentation of the Darwin-Wallace Medals at the special Meeting of the Society held in the Memorial Hall of the Royal Geographical Society on 15 July 1958, addresses were delivered by Dr A. Tindell Hopwood, F.L.S. and Dr A. J. Cain, F.L.S., to commemorate the bi-centenary of the publication of Linnaeus's *Systema Naturae*, Edition 10 in 1758. These addresses follow.

THE DEVELOPMENT OF PRE-LINNAEAN TAXONOMY

By A. Tindell Hopwood

The urge to classify is a fundamental human instinct ; like the predisposition to sin, it accompanies us into the world at birth and stays with us to the end. A child in its nursery sorting beads according to their colours, is doing essentially the same thing as the marine biologist sorting plankton in the laboratory. Moreover, their activities do not differ in principle from that of John Henry Newman when, seated before the fire in his old age, he burned the greater part of the accumulated correspondence of a lifetime.

That these three examples could be multiplied indefinitely suggests that a large part of human effort depends on the classification of objects. If this be so, there ought to be some general principle underlying what is, as we well know, a semi-automatic action. Now the child's beads are either red or not red ; the biologist's organisms are either zoaea larvae or something else ; and Cardinal Newman's letters were either worth keeping or they were not. In short, we are here dealing with examples of Excluded Middle, that any given object is either A or not-A.

This method of forming groups is sufficient for most ordinary purposes. To distinguish a horse from a cow, a sheep from a pig, or a cat from a dog is all that the majority of people require. Since any sane person knows what a horse, or a cat, or a dog looks like none of the groups needs definition : the distinctions are made by virtue of our experience of life. Everyday classification, then, is mainly dependent on two factors, our general knowledge and the law of Excluded Middle, and because biological classification is but a special case of a general rule there can be no doubt that in its beginnings the same two principles were employed.

Now any attempt to discover the course of a trend of mental activity that has its root very far back in human history speedily encounters two main obstacles, namely the scarcity of documents and the extreme difficulty of interpreting them. Both obstacles are present in full measure when we consider the foundations on which European science was built.

It is generally agreed, and for my present purpose is demonstrably true, that the study of science in Europe began with the Greeks, who were heirs to the thought and method of Assyria, Chaldaea and Egypt. There is also general agreement that the study of living organisms developed from the practice of the art and mystery of medicine.

Of Assyro-Chaldaean medicine we know very little, but the few documents that exist suggest that it was largely based on magical formulae directed to the expulsion of the evil spirits that possessed the patient, although some simples and minerals were also used with beer or fat as excipients.

Much the same is true of Egyptian medicine, but the Edwin Smith papyrus, which dates from about 1800 B.C., is evidence that side by side with the magic there existed a scientific tradition based on careful observation and, to some extent, on experiments. Moreover, internal evidence indicates that this scientific tradition went back to the early part of the third millenium B.C. None-the-less, there is no trace of any interest in a biological classification, neither is there anything that might be described as a nascent biology ; a result that is entirely in keeping with the severely practical, unspeculative nature of Egyptian thought.

The Greeks, whatever their origin, were far otherwise ; from the very

first they sought the ultimate causes of things. Thales of Miletus, the founder of the Ionian school, is an obscure figure in history who lived during the last third of the seventh century B.C. and the first half of the sixth. He seems to have left no writings of his own and what little is known about him depends entirely on the testimony of others. None-the-less, his successors looked up to him as the founder of Greek science ; as Aristotle wrote, ' he was the first to seek the material cause of all things ' (*Metaphys.*, *A*, 3). This is more than a bald statement of fact ; it implies that, as the first to seek a general principle underlying his observations, Thales was the first to use the method of induction.

The next landmark in biological history is also of Ionian origin, namely the Hippocratic corpus. It was written at diverse times by diverse hands in diverse places, but, though the whole is imbued with a spirit of observation and experiment, it does not make a specific contribution to the development of the theory of classification. For that one must look to Plato.

Plato, an amateur of mathematics, had very little interest in living things. Aristotle wrote, ' From his earliest years Plato was familiar with the Heraclitean doctrine of Cratylus, that all sensible things are in a constant state of flux and that we can have no knowledge of them. To the end of his life Plato remained loyal to those tenets— '; and, in the introduction to the *Timaeus*, Plato himself said, ' We must, then, in my judgment, first make this distinction : what is that which is always real and has no becoming, and what is that which is always becoming and is never real ? That which is apprehensible by thought with a rational account is the thing that is always unchangeably real ; whereas that which is the object of belief together with unreasoning sensation is the thing that becomes and passes away, but never has real being.'

Plato was not very interested in the thing that has no proper being. None-the-less, he made a contribution of the highest order to the theory of classification, for, in the *Sophistes* and the *Politicus*, he laid down the rules of dichotomy—sometimes called ' logical division '—and showed how by summing the differentiae one arrives at the definition of the species. He also showed how the definition of the species may vary according to the differentiae selected at each step in the dichotomy. He did not apply the method to biology, but used it in the two dialogues just mentioned to characterize both the sophist and the statesman.

Aristotle discussed and criticized the method in at least four of his major works, namely, the *Analytica Priora*, *Analytica Posteriora*, *De Partibus* and *Metaphysica*. Of these, the third is that most closely concerned with the history of Taxonomy.

The direction of the argument is made clear from the outset and, as the following quotations from the *De Partibus* show, Aristotle was upholding the claims of a natural classification, as he understood it, in contrast to the artificial systems to which dichotomies inevitably lead.

' Some writers propose to reach the definitions of the ultimate forms of animal life by bipartite division. But this method is often difficult and often impracticable.'

' Again it is not permissible to break up a natural group, Birds for instance, by putting its members under different bifurcations, as is done in the published dichotomies, where some birds are ranked with animals of the water and others placed in a different class.'

' If such natural groups are not to be broken up, the method of Dichotomy cannot be employed, for it necessarily involves much breaking up and dislocation.'

' The method then that we must adopt is to attempt to recognise the natural groups—each of which groups combines a multitude of differentiae, and is not defined by a single one as in dichotomy."

It so happens that almost all Aristotle's writings on Zoology have survived,

making it possible to judge how far he was able to observe the principles laid down in his criticisms of Plato. First of all, however, we must consider the arrangement of his works and the nature of the evidence on which his conclusions were based : we must also keep in mind that for Aristotle, as for the Greeks in general, the universe was something in orderly motion, possessing ' psyche ' or soul, and in some sense a living whole.

So far as the arrangement is concerned, the logical treatises precede those on natural history, and the latter those on metaphysics, ethics and the arts. Secondly, of the biological treatises, those dealing with the soul and the manifestations of the psyche precede the *Historia Animalium*, whereas those dealing with the functions of animals, their parts, their movements, the manner of their generation and so forth, follow it. This succession is neither accidental, nor is it wholly dependent on the whims of later editors. There is plenty of internal evidence to show that, broadly speaking, it reflects the order in which the books were written., So, for example, the *De Partibus* refers to the *Historia*, and the *De Generatione* to the *De Sensu*, clearly indicating the order of writing. A further consideration is that the treatises were written for use in the Lyceum ; one might almost call them Aristotle's lecture notes, and the order suggests very strongly that he regarded his system of logic as the foundation on which all else rested.

As for the nature of the evidence on which his conclusions were based it was of two kinds, namely ; what he knew from his own observations, and what he had learned by report. In neither instance do we possess the whole, for at least one important work, that on anatomy, has been lost. None-the-less, there is seldom much difficulty in distinguishing between the two types of evidence, neither can there be any doubt that when he had to rely on the reports of others he examined them critically before he accepted them. All this evidence, astounding in its quantity and variety, is dealt with in an orderly manner, yet nowhere is his system of classification formally set out. On the other hand, there is so much internal evidence that to recover at least the broad outlines is by no means impossible.

Overriding all else is the great division into those animals that have red blood and those that are bloodless, a division that corresponds almost exactly to our vertebrates and invertebrates. The former are then divided into the vivipara and the ovipara. The vivipara comprise our mammals, including the cetacea and man, and the latter the remainder of the vertebrates from birds to fish. The ovipara are then divided into those with perfect eggs (birds, reptiles and amphibia) and those with imperfect eggs (ovoviviparous reptiles and fish, as well as fish in general) ; a division which it is tempting to compare with our views on cleidoic and non-cleidoic eggs.

Continuing in this way and using a different character at each successive step, Aristotle constructed a *scala naturae* which was remarkably accurate considering the period in which he lived. It depended on the theory of subordination of characters, which determines that a given character is universal to those subordinate to it, but is itself particular to a higher category. For example, a species is universal to its sub-species and particular in respect of its genus.

This *scala naturae* has often been regarded as an early form of evolutionary arrangement, but this is a mistaken view. As I tried to show some years ago, a universe of Euclidean space with time as a discrete entity does not allow of an evolutionary process. Every event is precisely situated in space and time and no transition between them is possible. That the Greeks were aware of this is shown by the famous paradoxes of Zeno the Eleatic, who posed the problems of the arrow in flight, and of Achilles and the tortoise.

An even more potent factor existed in Aristotle's own thought. The point has been discussed at length by Abel Rey and by Léon Robin to whose

works reference should be made. Here there is only room for the barest outline.

The world was not created ; it has existed from all time in the harmony of its real qualitative relationships, which are real only because they are permanent (cf. Plato, *supra*). Even the changes that take place follow an eternal plan. Everything is based on the philosophy of the concept, and concepts cannot change because they are eternal.

After Aristotle there was a gradual decline. It began in the Hellenistic schools of Alexandria and culminated in the early Middle Ages. By then Aristotle was almost unknown in the West and even Plato was better known from the neo-Platonists of Alexandria than from his own writings. The rediscovery of Aristotle began about the middle of the twelfth century and by the beginning of the thirteenth century had become so dangerous in the eyes of the theologians that his works were formally proscribed by the University of Paris in 1210, 1215 and 1231. On the other hand, the University of Toulouse gave specific permission for their use in 1229 and, in 1245, was condemned by the Pope for its temerity. Taking this background into account, together with the influence of Plato, or at least of neo-Platonism, on the prevailing Augustinianism, it is not surprising that the scientists, among whom Robert Grosseteste, Bishop of Lincoln and Chancellor of Oxford University, is a well-known figure, devoted their time to physics rather than to biology. None-the-less, it was during this period that the importance of direct observation and experiment began to be recognized once again.

Of the few mediaeval biologists, Albertus Magnus (1206–1280) was the most important, because he was the first Christian to look to reason instead of faith for an explanation of natural phenomena. He turned to Aristotle for his physics and biology. His zoological observations are worthy of note ; for example, he was the first to describe the vocal sacs of frogs, but his services to taxonomy were small. Neither did his successors, whose work in Ornithology has been so admirably summarized by Stresemann do very much during the next three centuries to develop taxonomic science.

In so far as he required a system the great Conrad Gesner (1516–1565) leaned heavily on Aristotle, but his species were set out alphabetically in the mediaeval manner. Walter Charleton (1619–1707), physician to Charles II, followed the example of Aldrovandus and others in dividing the birds according to their habits : his main groups being thus arranged

1. Land dwelling species
   - (*a*) Flesh-eaters
   - (*b*) Insect-eaters
   - (*c*) Grain-eaters
   - (*d*) Berry-eaters
2. Water-birds
   - (*a*) Palmipedes
   - (*b*) Fissipedes
     - (i) Fish-eaters
     - (ii) Insect-eaters
     - (iii) Plant-eaters.

This was practically the last attempt to classify organisms according to Aristotle's principles : it was published in 1668.

Eight years later, Willughby's posthumous work *Ornithologia Libri Tres* was published by his close friend and literary executor John Ray (1627–1705) to be followed in 1693 by Ray's own *Synopsis Methodica Animalium Quadrupedium*. Ray is generally regarded as the author of the method of classification adopted in both these works, a method that was revolutionary in that it entirely disregarded Aristotle's principles and went back to the dichotomies of Plato, on occasion becoming trichotomous when that was more suited to the author's purpose.

Ray, one must admit, was a genius who displayed consummate skill in choosing his differentiae. Because of this his arrangement was as accurate as it

well could be considering the period when it was drawn up. It held its own for nearly a century, being used by some workers in preference to the Linnaean method. There were some whose preference was backed by reason, but there were others, and Pennant was among them, who objected to the Linnaean arrangement because man was therein associated with the monkeys. When Bishop Samuel Wilberforce protested against Darwin's views on human evolution, he was not being original, he was merely bringing Pennant up to date, and, dare it be said, had Huxley known that he might not have been so rude !

THE POST-LINNAEAN DEVELOPMENT OF TAXONOMY

By A. J. Cain

Mr President, My Lords, Ladies and Gentlemen.

You do me too much honour, Mr President, in asking me to contribute to so important an occasion ; moreover you have both placed before me perhaps the most biologically distinguished front row that has ever confronted a scared young man, and caused me to be preceded by the erudite and judicious discourse which we have just heard. Nevertheless, from obedience (or rashness) I will make the attempt, and you shall judge it.

Taxonomy at the present day is in a state that would have exhilarated and alarmed Linnaeus in equal measure. Mixed up with much excellent practice and many new developments (especially at the level of the species) is a considerable variety of opinion and diversity of practice relating to the basic principles of classification and nomenclature, the theory of which has been hardly examined at all. Much practice is intuitive instead of explicit and exact, and at least in this country most taxonomists learn their trade more by imitation than by proper professional training. This would have shocked Linnaeus. He knew perfectly well what his principles were. It is often said what a remarkable young man he must have been to have thought out the plan of all those amazing books at so early an age ; but in fact he was taught his principles at school. It is true that school lessons make little impression on many, and to Linnaeus belongs the very great distinction of applying with consistency the rules of classification he learnt in the study of logic to all three realms of Nature. To understand the situation at the present day, one must look a little at the history of taxonomic principles. Dr Tindell Hopwood has just told us how those of Aristotle were revived after the Dark Ages and became the taxonomic principles of European naturalists. Linnaeus, too, was an Aristotelian in his theory. I have surveyed his principles elsewhere (1958) and will only recall their main features.

The Linnaean taxonomy tried to analyse the things it classified, by the use of *Logical Division* and certain *a priori* ideas of what must be most important (and therefore must be used for the principal divisions) in both animals and plants. At first sight this seems wholly admirable—of course, we must classify things by their real natures, not by mere superficial resemblance. But to give one example, Cesalpino, using this principle, said that the most important thing a plant could do was feed (in order to exist at all) and consequently the first division of plants must be into woody (with a more perfect system of distributing food internally) and non-woody. The principal difference, therefore, was between herbs on one hand, and trees and shrubs on the other. After this, if there was a surplus, the plant could reproduce ; consequently the next division must be made on the reproductive organs and especially on the embryo which was the most essential of them all. By the time of Linnaeus, the first division was seen to be so unnatural that it was dropped ; but Linnaeus accepted—indeed

insisted on—the second as the basis of both his artificial and his natural classification. Even at the present day, we can hardly say that we understand the real natures (in any sense) of living things so well that we can classify by means of them, and in the past this seemingly reasonable attempt to classify by what things really are has opened the gateway for the whole of a man's philosophy of nature to enter his biological classification. The result makes interesting reading for the historian of science but is seldom as good taxonomy as it could have been without the influence of various wrong preconceptions. Dr Tindell Hopwood has pointed out the influence of physical theories on taxonomy (Hopwood, 1949). Here is the means by which they and so many others have exerted their influence.

The second characteristic of Linnaeus's taxonomy was that its primary category, as being most stable and memorable of all, was the genus not the species. All botanists (and by implication all zoologists and mineralogists) must know their genera, which must be separate, distinct entities with distinct names, and natural so that all would agree on their limits. This was necessary because in fact at the time the larger groups, of plants at least, differed violently from author to author, while the species were often very difficult to identify in any one genus, being mixed up with varieties of all descriptions, and themselves confused by bad diagnosis and doubtful synonymy. The genus was the most useful practical unit.

The third characteristic was that species, even if often difficult to name, were regarded as definite entities, clearly separable eventually from each other. As Mayr (1942) has put it, they were *static* (with no reference to change in time or space) and *monotypic* (so that a single description and diagnosis would suffice, if correctly drawn up, for every member of a species). I would add that they were also *genetically unitary*, each one normally breeding true, and *equivalent*, since there were no different sorts of species. Species were regarded as created separate and distinct, which was a powerful spur to their study ; but as Ramsbottom (1938) has pointed out, Linnaeus came to think in later life that some might have arisen by hybridization since the original Creation.

A fourth characteristic was that the best classification (except for the purposes of mere identification) was that which brought together those plants or animals that were really alike, that agreed fundamentally. These were those that had the greatest degree of affinity, and to put them together in this way was to follow out the plan of the Creator himself. Taxonomy could thus be regarded as a highly religious duty, and some of the animosity raised against Darwin in the breasts of trained naturalists might be traceable to a feeling that he was attempting to degrade their noble profession.

A consequence of the special creation of species was that they were considered to be perfectly fitted, or very nearly so, for the role assigned to each in the Economy of Nature. Darwin's two most telling pieces of evidence for evolution were aimed directly at this belief—namely the difficulty of explaining vestigial organs, and the discrepancy between the taxonomy and ecology of animals on one hand and their distribution on the other.

Linnaeus's reference system was a nomenclature very well fitted indeed to his theory of taxonomy. The construction of a reference system must always be done in relation to what must be referred to. His principal referendum was the genus, his second the species. His generic name, therefore, was a single word (and so most concise and memorable), diagnostic if possible, or dedicatory, or at all events not altered by discoveries of new genera, and therefore as stable as possible. To be memorable, it must also be distinct, and meaningful ; and moreover, it had to be acceptable to *educated* men. Many times he says, we must not commit solecisms or we shall be laughed at ; we must obey the laws of grammar and euphony or the masters of rhetoric will soon find arguments

to attack us ; we must not continually coin names ending in *oides*, because not only do they suggest intermediates between genera, which is unthinkable, but people will say on looking over a treatise full of them, ' The author is no botanist but a botanicoides!'

The specific differentia, on the other hand, was a mere qualification of the generic name, diagnostic only by differentiation within the genus and so liable to alteration whenever new species belonging to that genus were discovered. For brevity, the differentia should never exceed 12 words. Later, for convenience in quick reference he invented trivial names, which were single words like the generic names. This invention of binominal nomenclature is what he is particularly remembered for to-day, but I must point out that Linnaeus, the arch-rule-giver, the meticulously precise man who reduced everything to rules or aphorisms, thought so little of his trivial names that it was left to his pupils to make rules for them. What he did make rules for with great care, was the generic name and the differentia ; he laid down their construction, alteration (if inappropriate or ignorantly formed), synonymy, and—most important—their homonymy, so that no name should refer to more than one entity.

The suitability of such a system of nomenclature for Linnaeus's own taxonomic theory, with its genera, distinct and memorable, as primary units and its species, equally distinct in theory, as subdivisions of the genera, is obvious.

Between Linnaeus and Darwin stretches a period of change which is perhaps the most interesting in the whole of pre-Darwinian taxonomic history. It is characterized by the gradual abandonment of *a priori* reasoning, for the excellent reason that whatever organ system was taken as necessarily the most important, some group or other formed in accordance with it was demonstrably unnatural—as unnatural as the division into woody and non-woody plants that separated the laburnum from the clovers. According to Sachs (1906) the abandonment was achieved by Linnaeus ; but this is wrong. It was hardly achieved when the theory of evolution arrived to complicate matters. (Indeed it is arguable that it would have been an excellent thing for the development of taxonomic theory if evolution could have been held off for 50 years.)

The principal group of taxonomists in this period who set out their theories of taxonomy was Parisian, and included Lamarck, Cuvier, de Candolle, Geoffroy St Hilaire and others. Their fundamental agreement on the principles of taxonomy is seldom if ever referred to by historians of science, who fasten on the less fundamental disagreements between them, but it is clearly shown by comparison of their writings (Cain, 1959). A quotation from Cuvier's preface to the *Règne animal* will serve to indicate their ideas. He says that we have many things to deal with in classification, and we need a hierarchy of divisions, a *method*, so that only a few things need to be compared within any one group and only a few characters need to be examined. (Linnaeus said the same, but used the word *methodus* quite differently.) ' But,' Cuvier goes on, ' when the method is a good one, it is not limited to teaching names. If the subdivisions have not been established arbitrarily but are based on fundamental relationships, on the essential resemblance of things, the method is the surest means of reducing the properties of these entities to general rules, of expressing them in the least terms, and of engraving them readily on the memory. To make it such, an assiduous comparison of entities, directed by the principle of the *subordination of characters*, which itself is derived from that of the conditions of existence, is used. Since the parts of an entity must have a mutual conformity, there are some traits of conformation which exclude others (for example a hoofed animal cannot be a carnivore since it will be unable to seize its prey) ; there are some which on the contrary necessitate others ; therefore when such and such traits are known in an entity one can calculate those which coexist with them, or those which are incompatible with them.

The parts, properties or traits of conformation which have the greatest number of these relations of incompatibility or coexistence with others, or in other words which exert the most marked influence over the ensemble of the entity are those which are called the *important characters*, the *dominant characters* ; the others are the *subordinate characters*, and there are thus different degrees of them. This influence of characters can sometimes be determined by reason, by a consideration of the nature of the organ. When this is not possible simple observation must be used, and a sure means of recognizing the important characters which derives from their very nature, is that these are the most constant, and that in a long series of diverse entities, grouped together according to their degree of likeness, these characters are the last to vary. From their influence and their constancy equally results the rule, that they should be preferred for distinguishing the great divisions, and that in proportion as one descends to lower subdivisions, one may descend also to subordinate and variable characters.' This procedure, he says, should lead us straight to the only perfect method, the *natural* method, in which all resemblances and differences are taken into account.

This quotation shows what a curious mixture of different bases for classifying was being used. The arrangement of a group is viewed (i) as a means for summarizing the available information about it and its subgroups in as few and concise generalizations as possible, but also (ii) as the results of dividing the group on the accepted physiology of the day (this indicating what organ system has the greatest influence over the whole body), and (iii) as the results of grouping together those forms most like each other, using the characters *observed* (not deduced) to be the most constant. In fact, the procedure was in part at least a vicious circle. The constancy of a character can only be observed within groups which are natural ; consequently these groups must be formed before any particular characters can be checked ; and indeed much of the so-called physiological theory was actually based on simple observation of what organs or organ systems varied least in already-constructed groups. The really sound basis of the classifications produced at this time in France was the excellent anatomical observation made on many species and groups ; and the procedure was in fact empirical, by grouping species together, not *a priori* by division on set principles.

There was, however, one man who realized the necessity for an empirical approach to the natural classification, undisturbed by any considerations of what ' must be ' most important ; this was Adanson. Rejected by the other French workers precisely because he did not use an *a priori* approach, grossly misunderstood by some historians of science, he is at last beginning to receive his due recognition. The importance of the purely empirical approach, as Darwin himself says in the ' Origin ', was only just being realized nearly a century after Adanson. Any character whatever, however fundamental it may seem, however constant through large groups and therefore prominent in their diagnosis may be found suddenly to vary in one form which on all other characters is a good member of one of these groups. The nudibranch *Calma* feeds on the yolk of fish eggs and does not need to defaecate ; it has no anus, but it is not therefore removed to the Platyhelminthes or Coelenterata—yet the lack of an anus, of a one-way gut, is a ' fundamental ' character of these great groups, in the sense that all members of them show it. Similarly, the Protozoa are defined as unicellular (or non-cellular) animals, and this is a fundamental difference between them and the rest of the Animal Kingdom. But in fact such a form as *Zoothamnium* can be called a colony of unicellular (or acellular) individuals only if a buttercup plant is also to be called a colony. Again, the Coelenterata have no anus, but *Cerianthus* has what is always described as a ' terminal pore '. I do not venture to assert that it is in fact an anus and used as such,

but I wonder whether the expression ' terminal pore ' has never been used to save the definition of the group. It is arguable that every character which has been taken as fundamental to the production or existence of any great group of living things is fundamental only to its definition, and in fact is not both common to and peculiar to all the members of that group. By the time of Darwin, such examples as those given, the significance of which for a natural classification is still only partly appreciated, had brought about the downfall of *a priori* schemes of classification.

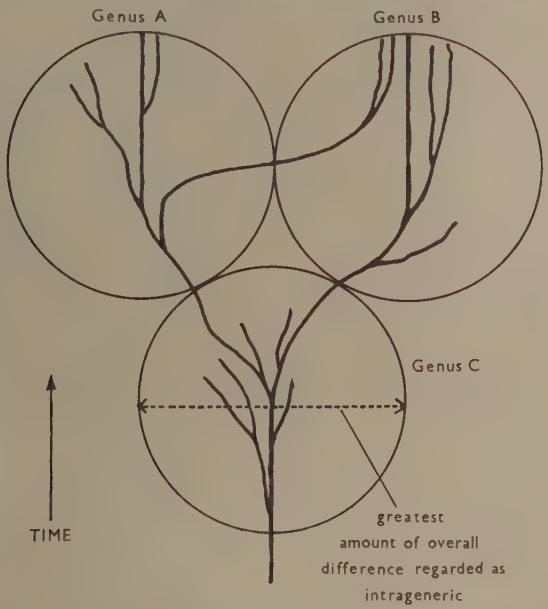
Meanwhile, all that need be said about the progress of nomenclature is that it was nearly always binominal, and thus closely followed the Linnaean system, but lack of agreement on various points of synonymy and homonymy, sometimes in active opposition to some of Linnaeus's rules, had produced something very like chaos. Different systems were used by different people and in different countries, and the major purpose of nomenclature (to provide an international reference system) came near to being defeated. On top of all this arrived Darwin and the theory of Evolution.

Darwin as a taxonomist has been very seldom appreciated. He was in fact a good one, and the first part of the thirteenth chapter of the ' Origin ' (the fourteenth in the later editions) is an important document in taxonomic theory (Cain, 1959). He inherited a classification of animals and plants made on the very mixed basis described above. What did he do?

First, he explicitly rejected all ' physiological ' and indeed all *a priori* reasoning of any sort, commending A. St Hilaire for doing the same, and giving excellent reasons for the rejection. As a consequence he asserted that the taxonomic importance of a character depends only on its co-variation with other characters within natural groups. He admitted, rather unenthusiastically, the utility of a natural system as producing useful generalizations, but said that there was more to it than that ; these natural resemblances are due to descent with modification. This means that he equated the natural classification, based on the overall resemblances and differences of the forms observed, with a phylogenetic tree ; and more than that, he said that those characters which, so far as we can judge, are least likely to be modified in the course of evolution are the most important, thereby bringing in again an *a priori* basis for classification. Both these propositions are dangerous.

In the first place, Darwin was not clear about the relation between a phylogenetic classification and a natural one (in the sense used at the time, i.e. a classification in which all characters are taken into account, not one in which the groups were based on the natures of the entities as determined by *a priori* reasoning). In the ' Origin ', Darwin was especially interested to produce a cause for divergence, and says very little about convergence, of which, of course, he knew and discussed many good examples. Now it is true that if nothing but divergence has gone on in a group, a natural and a phylogenetic classification of it will virtually coincide. But if convergence has occurred, especially in a group for which we do not have a good fossil record, a natural classification may well associate together two convergent forms ; indeed we may be wholly unaware that we are dealing with convergence at all. It is true as Darwin said, that since one is most like one's immediate ancestors and descendants, a truly phylogenetic system will bring together those forms most like each other. But, as Mr J. S. L. Gilmour and Dr S. M. Walters have pointed out to me, although a phylogenetic classification may be a natural one in that sense, even in a group with a perfectly known fossil record it may separate things which are indeed on overall similarities very alike because of convergence. The figure illustrates such a case. On overall similarity, i.e. on a natural classification, the genera should be as shown by the circles for the evolutionary dendrite in question, and the natural genus B is then diphyletic.

In general, we can say that the natural and the phylogenetic systems of classification are not the same, although their results may often coincide. Moreover, in most groups, for which we do not have a good fossil record, classification can only be natural, and a great deal of convergence may well exist which is inevitably hidden from us by this method of classification. This is a difficulty which has never been faced. Allied to it is the problem of reconstructing the common ancestor of a group. This is likely to have had many if not all of the characters common to the group as known at present, so the usual procedure is to construct it from such characters. But this leads to an archetype, or an embodied definition, rather than the actual ancestor. A similar procedure for obtaining an intermediate ' ancestor ' was once carried out as a severely practical



exercise—in the construction of Piltdown man which was made up as a mosaic of characters belonging to the known great apes and modern man ; but subsequent discoveries of actual fossils merely made it seem more and more anomalous.

Of Darwin's *a priori* proposition that those characters least likely to be modified in the course of evolution will be the most important taxonomically, all one can say is that no doubt in general it is right, but without a good fossil record, who can say which they are in any given group? I believe that many missing links are missing only because on these procedures they cannot be recognized for what they are, and some hypothetical conception is being awaited, naturally with disappointing results.

The outstanding nomenclatural feature of Darwin's time was the beginning of attempts to draw up a set of rules which should be internationally acceptable. This was actually achieved in the modern period of taxonomy, which may be dated from about 1900. The device accepted for producing stability was that the first valid publication of a given name should have priority over all subsequent ones. This, in theory, was a good piece of legislation because instead of the best name or the most widely used, both of which it might be impossible to get

agreement on, the criterion used was of actual dating which should be ascertainable without doubt. Unfortunately it threw the whole emphasis of nomenclatural research back on to the earliest and least satisfactory literature, and stability was rather lost sight of in the resulting confusion. But the other and greater feature of the modern period of taxonomy, is that just as a Linnaean system of nomenclature was at last generally accepted by international agreement, the only remaining Linnaean element in taxonomic theory collapsed completely—the species as an entity of classification valid in all groups of living things disappeared. The nomenclature had arrived approximately a hundred years too late. Up to Darwin's time (and much later in the less studied groups) the practice of logical division, which made the genus so important, was gradually giving way, and the genus at present (Cain, 1956) is merely a collection of species, natural only in a very restricted sense, not a special distinct entity subdivisible into species. In the last 30 years it has been realized that neither asexually reproducing forms nor forms which are part of a single phylogenetic lineage can be called species in the same sense as can sexually reproducing forms which co-exist but do not interbreed. Even in these, an appreciation of the facts of geographical variation has produced the categories of the subspecies and superspecies, and a further difficulty in defining specific limits. I have reviewed this topic elsewhere (Cain, 1953). It is the outstanding contribution of what Huxley (1940) has so happily called the 'New Systematics'. I do not propose to discuss it here for two reasons—first it is or should be well known to everybody, and secondly, it is already old. In saying that, I am paying it the greatest compliment possible, since it is old not by supersession, but by general acceptance. The work of such men as Turesson, Danser, Babcock, Clausen and Turrill amongst the botanists, and Kleinschmidt, Hartert, Rensch, Jordan, Poulton, Simpson, Mayr, Huxley and Dobzhansky among zoologists, to name only a few, is already classic. The new systematics is no longer a growing point. It is rather a splendid flower which will soon run to seed—and then be truly fruitful.

One seed which has already started to germinate is the taxonomic treatment of the species. As a result of the new systematics, the sexually reproducing species has achieved a new role among zoologists as the single evolutionary unit, evolving in genetic independence of all others, as Dobzhansky (1951) and Simpson (1951) in particular have emphasized. Consequently there is a strong movement among zoologists to employ the internationally recognized binominals to such units. But the proper application of specific names is far from certain when we are dealing with ecotypes, ring species, superspecies, and all asexual forms. Several botanists are so impressed with the enormous complexity of evolution at the population level that has been revealed by the new systematics, that they have proposed to leave the species as merely the lowest category in a mainly morphological classification, a map of the general diversity of the animal and plant kingdoms which will certainly always be of use. They wish the units of experimental and evolutionary taxonomy to be wholly separate from those of orthodox taxonomy. Camp has argued this case with great force, and a most interesting and well designed terminology based on the root *deme* (unfortunately incorrectly given in some text-books) has been put forward by Gilmour & Gregor (1939) and developed by Gilmour & Heslop Harrison (1954). Gilmour (1951) wishes to go back to the older idea of a natural classification as one which stores the maximum information as generalizations (which certainly is the only one we can use for most groups since the fossil record is in general so poor) and leave the species as the lowest category in it.

This raises a most important point. A reference system, as I remarked before, is always constructed in relation to what one wants to refer to. Linnaeus wanted first to refer to small natural groups which could be held stable while

better classifications of them were being worked out. These were his genera. But he also wanted to refer to the separate true-breeding units within them by distinguishing these from each other and relating them to the genus, which every biologist should already know. So he produced his specific differentiae. Now, what exactly is it that one wishes to refer to, which requires an internationally agreed nomenclature because the same references must be made everywhere? This is a point that must be discussed and settled. Biospecies as distinct units of evolution are very likely to be referred to by zoologists everywhere ; on the other hand it is quite possible that botanists in general will wish to leave many of their evolutionary units outside the official nomenclature. Certainly it seems likely that either there will be considerable diversity in the significance of binominal names in different groups of living things, or else some standard usage will have to be agreed upon.

What else can be said about the future? I would prefer to be like the Irishman and prophesy after the event—one is so much more likely to be right. But I think there are certain trends that can be recognized even now. I think that we are about to see a considerable revision of the whole basis of taxonomic theory, especially the relation of phylogenetic to natural classifications, a clear separation of phylogenetic weighting from recognition of the co-variation of characters, and the development of methods for making comparisons, whether phylogenetic or natural, more precise. These will require a strict definition of the single character, the recognition of the ' same ' character in different forms, and some method of making overall comparisons. It is a sure sign of a swing of interest towards these subjects that in the last 14 months papers on them have appeared from three independent sources. Sokal & Mitchener (1957, 1958) in the U.S.A., Sneath (1957 a, b) in London, and myself and G. A. Harrison in Oxford and Liverpool (1958) have all attempted to devise procedures for making taxonomic comparisons more precise. This sort of quantitative approach to comparison will, I think, be specially characteristic of what I venture to call the New Taxonomy. Mayr has truly said that the number of characters of an organism is limited only by the patience of the investigator. But we now have powerful mechanical (or rather electrical) aids to patience, and the electronic computer has already, and for the first time, been employed in making very large numbers of taxonomic comparisons by Sneath. The development of precise procedures and the employment of non-human computers allows us to hope at last for the development of a taxonomic hierarchy with a really quantitative basis, a properly surveyed map of the diversity of living things instead of a highly subjective and distorted sketchmap. The basis of these procedures is likely to be Adansonian. With the more general recognition of how much classification must—for lack of information—be entirely natural, there will be far less enthusiasm for the multitude of so-called phylogenetic trees that were produced so feverishly last century, when over-eager supporters of evolution thought that their case was incomplete unless every known form could be hung on some twig of the tree. It will be realized, for example, that the sort of arguments put forward by Hadzi (1953) for his particular arrangement of the Platyhelminthes, Ctenophora and Coelenterata are on just the same logical footing as, and have no more validity than, the arguments used to uphold the more traditional arrangement. Hadzi's work has great value at the present day, in reminding us how much (and how unjustifiably) under the spell of Haeckel are nearly all our text-books of systematic zoology. But the alternative interpretation put forward is only one of many possible ones, and there is no way whatever of choosing between them without a good fossil record, which we have not got. To a large extent, it does not matter. One can talk about the Protozoa, for example, very usefully as a group, although it is almost certainly polyphyletic. The Ciliophora, in their possession of cilia and transverse fission,

resemble the smaller Turbellaria closely. Moreover, in conjugation each individual both gives and receives a gamete, and so could be considered hermaphrodite. The Ciliophora may well be reduced from a Turbellarian ancestor.

Together with these changes in taxonomic theory, there will inevitably be a considerable change in nomenclature (although the history of the last 200 years suggests that it may make headway very slowly). No relevant studies have yet appeared on the theory of reference systems in general, and so far as I know, the only instruction in nomenclatures *in general* that is given in this country is comprised in some lectures of my own at Oxford. But without a sound basis in theory and some knowledge of different practices, one cannot make any other than blind and *ad hoc* alterations to the present systems of biological nomenclature. Some progress has been made already, nevertheless. It was a notable advance when the type specimen became regarded as only a name-bearer, and not in any sense necessarily typical. It is a great relief that the fantastic attempts to correct names according to classical spelling (a return, in effect, to Linnaeus's desire to appear erudite, and not, as he said himself, a backward provincial) have not been accepted. But stability is still too difficult to achieve, and in particular is threatened by the now wholly anachronistic system of requiring a generic name before a species can be named at all. The genus no longer has the importance that it once had. It is no more and no less useful than other ranks in the taxonomic hierarchy—the family or order, for instance. But it is not in the least necessary that the name of the order or family should form part of the name of a species, nor is it necessary that the generic name should. Moreover, there are good reasons why it should not. The necessity for putting a species into a genus before it can be named at all is responsible for the fact that a great deal of uncertainty is wholly cloaked and concealed in modern classifications. I should judge that even in the birds, the group with the most refined systematic arrangement at the level of the species and subspecies, at least one-quarter of the genera used in the classification of passerine birds are worthless. They are meaningless additions to the names of species of wholly uncertain place, and effectively hide the fact that the affinities of these species are not known. Genera are also great temptations to show off specialist knowledge. In consequence many groups have been grossly oversplit. The so-called order *Psittaciformes* (the parrots) would be a single genus in almost any other group of animals. Moreover, in many branches of zoology, the generic name is quite unnecessary. The physiologist who is interested in the concretion kidney of a snail, for example, must identify his material so that others can verify his work and attribute his results to the right sort of animal, but he is not in the least interested in the relation of that snail to other species in that genus. If he is working with *Helix aspersa* L. he is not concerned that *H. pomatia* is a member of the same genus according to some authors but is separated by others. Taxonomists have forgotten for too long that they are the name-makers for all zoologists and botanists, pure and applied, and are under obligation never to impose an unnecessary burden on others.

It seems clear that with the great alteration in the status of the genus since the time of Linnaeus, an effectively uninominal system will come into use. This will then be free from that part of the present instability caused by generic changes, which are mostly matters of opinion, and from the inclusion of a particular way of classifying things in the names of the things to be classified. The last traces of those unwarranted preconceptions that have bedevilled taxonomy since Aristotle will then have been removed. The necessity for avoiding homonyms and synonyms will, of course, remain as before, since it is a character of all reference systems.

It is desirable that the reference system for living things should be designed only for the purpose of reference to them ; but it is likely that any reform will

involve a modification of the present system rather than a complete break. It would not be at all difficult to design such a modification to convert the system into a uninominal one. If the generic and trivial name of a species which were used in the last revision after a certain agreed date, or which were first validly published for that species, were hyphenated together, the resulting combination could then be regarded as stabilized for ever nomenclatorially, and published as such. The combination, once given in full, could always be abbreviated for ordinary use, e.g. by shortening the first word to an initial. Anyone could then put what generic name they liked in front of it without altering it, and anyone who was interested only in identification could simply use the combination without any separate generic name. A detailed suggestion will be published elsewhere.

How will such reforms as these come about? It is unlikely that they will come from any body devoted to the preservation of Linnaean nomenclature, or from any of the larger taxonomic institutions, in which the staffs, far too small, are weighed down with the pressure of routine identification—an extremely valuable activity and of great importance to the State economically, but most inadequately provided for. Moreover, a certain training in taxonomic theory is necessary to handle the problems involved. Is it not extraordinary that young taxonomists are trained like performing monkeys, almost wholly by imitation, and that in only the rarest cases are they given any instruction in taxonomic theory? A casual apprenticeship or purely amateur approach (especially valuable though the latter is in certain circumstances) is not sufficient when highly technical matters are under consideration. For this same reason, it seems unlikely that much will be done by means of International Congresses. As H. G. Wells pointed out in 1900, a general election of the truth half-yearly (or at any other interval) is absurd. There are places where democracy is inapplicable, and this is one of them. If we are to be scientists, we must study the fundamental principles of taxonomy, must make those alterations in our practice which follow as consequences from them, must know in short what we are doing, for what purposes we are doing it, and how to do it efficiently.

Yet it is still true that even in the Universities one meets highly educated biologists whose only mental image of the taxonomist is of a man who can tell you the name of any animal or plant that anyone cares to bring to him—and if he can't, he is not much of a taxonomist. We must begin by educating our colleagues. Linnaeus, I believe, had the same difficulty. Let us hope that we shall have the same success.

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THE JOINT CONVERSAZIONE AT BURLINGTON HOUSE

THE PRESIDENTS of the three Societies :—Sir Cyril Hinshelwood, The Royal Society ; Dr C. F. A. Pantin, The Linnean Society ; and Dr C. J. Stubblefield, The Geological Society received the guests in the Royal Society's Council Room.

EXHIBITS

1. A live giant tortoise from the Galapagos Islands.

2. Nomination papers and certificates of Darwin and Wallace as candidates for election to the Royal, Linnean and Geological Societies.

3. A model of a 10-gun brig of the *Cherokee* class, representing H.M.S. *Beagle* as built.

4. A drawing of H.M.S. *Beagle*, showing the alterations made in fitting her out for her voyage of exploration.

5. Log of H.M.S. *Beagle*, 1831–37.

6. Charts illustrating the voyage of H.M.S. *Beagle*, together with some framed original charts, and other material relating to the voyage.

7. Some coloured photographs of drawings made by the artist Martens, during the *Beagle's* voyage.

8. Photographs of vegetation and localities visited during the voyage.

9. Specimens of Darwin's finches and skins of marine iguanas, from the Galapagos Islands ; specimens of fossils collected by Darwin in Argentina.

10. ' Insect problems Darwin may have met in Brazil '; material obtained by Dr H. B. D. Kettlewell when visiting some of Darwin's collecting areas in 1958.

11. Plants seen by Darwin during the voyage of H.M.S. *Beagle* which have since been introduced into cultivation.

12. Darwin's barnacles, an exhibit illustrating aspects of the biology of barnacles which touch on Darwin's interests, by Dr D. J. Crisp, Dr A. J. Southward and Mr A. P. Austin.

13. Books and other publications by Darwin and Wallace.

14. Darwin's geological notebooks.

15. A portrait of Darwin by John Collier.

16. Various other portraits of Darwin.

17. A portrait of Wallace by S. W. Beaufort.

18. A portrait of T. H. Huxley by John Collier.

19. Living and preserved specimens of insectivorous plants and of orchids, mentioned by Darwin in his books on these two subjects. Exhibit prepared by Mr R. A. Graham and Mr R. Harley.

20. Photographs of the unveiling of a memorial plaque to Wallace at ' Old Orchard ', the house at Broadstone, Dorset, occupied by him from 1902 until his death in 1913.

21. Some letters from Wallace.
22. Various photographs of Wallace.
23. Some Darwin *personalia*, including :—
    (*a*) a pipe with a stem made from the tibia of an albatross ;
    (*b*) two silk handkerchiefs ;
    (*c*) a botanical collecting box ;
    (*d*) a floral spray made from parrot's feathers brought from South America by Darwin as a present ;
    (*e*) parts of Darwin's microscope.
24. Wallace's Amazonian voyage ; notebooks, sketches and a sextant.
25. Examples of Wallace's collections, including Lepidoptera and a snake skin from Malaya.
26. Two manuscript pages of the *Origin of Species*.
27. Letters from Darwin to members of his family.
28. Portraits of members of the Wallace family.
29. Portraits of members of the Darwin family.
30. Frames of seeds, collected and arranged by Darwin.
31. A selection of letters from Darwin.
32. Paintings of Down House.
33. ' Genetical studies in some British plants '; an exhibit prepared by Dr W. B. Turrill and Mr E. M. Marsden-Jones.
34. ' The study of evolution by observation and experiment ';
    (*a*) Stabilization and rapid evolution of a multifactorial Character ; an exhibit prepared by Dr Ford, Mr Creed and Mr McWhirter.
    (*b*) Polymorphism and selection ; an exhibit prepared by Dr Ford and Dr Sheppard.

ACKNOWLEDGMENTS

The Societies' Darwin-Wallace Centenary Celebrations Committee gratefully acknowledged the help it had received from :—

Lady Barlow, for exhibits 23 (*b*) and (*d*), 26, 27, 32 and part of exhibit 29 ; Sir Charles Darwin, for exhibit 4 and part of exhibit 29 ; Col. Richard Meinertzhagen, for exhibit 23 (*a*) ; Dr Hamshaw Thomas, for exhibits 16, 21, 23 (*e*), 30 and parts of exhibits 13 and 31 ; Professor C. E. Tilley and the Department of Mineralogy and Petrology, University of Cambridge, for exhibit 14 ; Mrs E. C. Wallace, Mr A. J. R. Wallace and Mr R. R. Wallace, for exhibits 22, 25, 28 and part of exhibit 13 ; The Hydrographer of the Navy, for exhibit 6 ; The Rector of Imperial College, for exhibit 18 ; The Deputy Keeper of the Records, for exhibit 5 ; The Director of the Royal Botanic Gardens, Kew, for exhibit 11 and for material used in exhibit 19 ; The Trustees of the British Museum, for exhibits 9 and 17 ; The Trustees of the National Maritime Museum, for exhibit 3 ; The President and Council of the Royal Geographical Society, for exhibit 8 ; The President and Council of the Zoological Society of London, for exhibit 1 ; The Editor of the *Bournemouth Daily Echo*, for exhibit 20 ; The Editor of the *Geographical Magazine*, for exhibit 7 ; Messrs Neville Brown and Co., for the loan of the viewers used in exhibit 10.

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